THE INFLUENCE OF FORESTS ON THE QUANTITY AND FREQUENCY OF RAINFALL.

THE effect of the growth of forests on rainfall is receiving fresh attention in the Philosophical Society of Washington. At a recent meeting of that society Professor Fernow of the Department of Agriculture gave an historical review of the experiments made in Europe during the present century to determine the influence of forests on the quantity and frequency of rainfall. When the reading of Professor Fernow's paper was concluded, there was no time for the discussion of it, and a symposium on the subject was arranged for the next meeting. Mr. Henry Gannett opened the discussion, and his address was the most notable of the evening. He was followed by others, notably by Gen. A. W. Greely, who has made this subject a special study. That part of Professor Fernow's paper relating to the experiments in Europe are given here, followed by Mr. Gannett's remarks in full.

Professor Fernow's Paper.

I wish to call attention to the latest, most important, in fact the only scientific work, it seems to me, which has been done to establish the important but difficult question of the influence of forests on precipitation. I refer to the work of Dr. F. J. Studnika, professor of mathematics at the University of Prague, published under the title 'Basis for a Hyetography of Bohemia,' in which the results of many years of observation at 700 ombrometric stations are embodied, critically sifted, and scientifically considered.

The work of ombrometric observations, although begun in Bohemia during the last century, was newly organized in 1879 or 1880, when a systematic net of ombrometric stations was instituted; and in 1885 and 1886 it was extended over 700 stations, for the purpose of obtaining accurate data of the quantity and distribution of precipitation over the kingdom. Uniform ombrometers were used and very carefully placed. As at present organized, there is one station for every seventy-five square kilometres (about thirty square miles). No other country, I believe, can boast of such a service. Although the time of observation at most stations has been short, and the average would have been more accurately represented by an extension of observations for ten or twelve years, yet the last four years of observation, for which all stations furnish data, according to the author, represent two extreme and two average years, and are therefore quite useful.

The very large mass of material permitted a sifting-out of doubtful observations without impairing the number available for the construction of a rain-map of Bohemia, showing by isohyetal lines seven rain-belts or zones. The zones are so arranged that the lowest shows less than 500 millimetres rainfall, the three following differ by 100 millimetres each, the fifth and sixth by 200 millimetres, and the seventh by 300 millimetres; showing, therefore, a rainfall of 1,200 to 1,500 millimetres.

The central basin divides itself into halves by a line from north to south, running somewhat east of the middle Moldau, crossing the Elbe near the mouth of the Iser, and following the latter river; the western half showing the smaller amount of precipitation, namely, 500 to 600 millimetres; the eastern, with 600 to 700 millimetres, continuing in a small belt along the foot of the Erzebirge and the Boehmer-wald, encircling the first zone.

The other isohyetal lines do not embrace continuous areas, but follow in small belts the trend of the mountains. The larger amounts of precipitation are found in belts or islands in the higher altitudes of the mountains which surround this great basin. The continuity of the zones is much interrupted, so that it would be difficult to describe it without a map.

The maximum rainfall with over 1,200 millimetres is observed in the south near the sources of the Moldau and Wotawa; in the north, near the sources of the Elbe, Iser, and Aupa, on the range of the Schneekoppe. In regard to the distribution through the months, the experience has confirmed, that, with increasing absolute height, the winter precipitation increases in greater proportion than that of the summer, while those of spring and autumn are nearly equal.

Sufficient material was on hand from which to calculate the influence of altitude on the increase of precipitation, although for altitudes above 500 metres the material is not considered sufficiently accurate. Yet the general law is well shown, that with the altitude the quantity of precipitation increases in a retarded progression. This progression is calculated by forming altitude zones from 100 to 100 metres, grouping the stations in each, calculating the mean elevation and also the mean annual precipitation as observed for each class; then, by dividing the difference of precipitation in two neighboring zones by the difference of altitude, the amount of precipitation which corresponds to each one metre of elevation within that class is found. With this figure, the average amount of rainfall which theoretically belongs to each station according to its absolute elevation can be approximated by adding to or subtracting from the mean precipitation of the class as many times this amount as the actual altitude differs from the mean.

A single example will make this clear. Tetschen, for instance, is situated 150 metres above the sea-level. According to the table, the average elevation of 13 stations of the lowest zone, to which Tetschen belongs, is 182 metres, with an average precipitation of 506 millimetres. Now, as Tetschen has an elevation of 32 metres lower than the average, the rainfall should be $32 \times .79 = 25.4$ millimetres less than the mean of the class; hence, theoretically, according to its altitude, the quantity of rainfall for Tetschen should be $506 - 25.4 = 480.6$ millimetres; that is, 248 millimetres less than that actually found in an eight-years' average.

By using the figures for the two extreme zones and dividing by 100, the mean increase of precipitation for every 100 metres elevation is found to be 69 millimetres.

And now comes the application of this method to our proposition. The author argues that if the actually observed rainfall differs considerably from the theoretical, this is an indication that special agencies are at work.

He finds now, that, of 186 stations which he subjects to scrutiny (those offering the longest and most trustworthy observations), 48 show a considerable difference between the observed and the theoretically expected rainfall, and he finds also that these stations are situated in the most densely wooded portions of the kingdom.

The increased rainfall at the 48 stations is so considerable, that sufficient quantity may be ascribed to other local causes, as, for instance, height and form of a mountain-range in front or back, etc., without losing significance. Besides, the greater amounts of rainfall at these stations have been used in calculating the averages for the altitude zones, magnifying, therefore, these averages so that the actual difference between the calculated quantity and the actually observed one appears really smaller than if the quantities from deforested and forest areas are compared.

Expressed in percentages of the height of precipitation, an increased rainfall is shown for several localities in very large quantities, which will allow considerable reductions for other influences without losing their significance for the main proposition.

Especially important appears the fact relating to two stations near the rain minimum, which also shows this influence of the forest.

Lastly, as a matter of interest, I may state that the water balance is drawn for the whole kingdom, which is of special value, because the political boundaries coincide with those of the upper Elbe watershed; therefore it is easy to determine how much of the yearly rainfall is removed by the natural water-courses. According to the calculations made for the various zones by addition, the total precipitation upon the area of 51,955.98 square kilometres (about 20,000 square miles) of the kingdom is found to be 35.398,-670,000 cubic metres, of which the Elbe carries about one-quarter, or ten cubic kilometres, to the sea. This figure represents a mean rainfall for the whole country of 681 millimetres, while the mean observation is 693 millimetres.

In conclusion, allow me to say that I believe neither of the methods employed will alone be sufficient to investigate such a complicated relation in its generality as that which they try to establish or refute. All of them, modified and provided with such safeguards as will exclude the many disturbing influences, will have to work together towards a solution of the question.

Mr. Gannett's Paper.

At its last meeting, the society was favored with a very interesting and important paper by Dr. Fernow, in which there was presented a *résumé* of certain investigations made in this country and

Europe, concerning the supposed influence of woodland upon precipitation.

While criticising the methods used in these investigations, Dr. Fernow did not, if I understood him right, give his own views upon the main question.

The question is, Does the presence of woodland influence rainfall, does its increase increase rainfall, and does its destruction reduce it? I know no theoretical grounds upon which an affirmative belief can be based.

While the question is an interesting one to science, it is also a particularly important economic one to this country. The future value of a large part of our arid region, and our policy in regard to it, depend upon the decision. If the presence of woodland increases rainfall to an economic extent, we should begin at once to plant trees all over our Western plains, and supply them with water until they in turn supply themselves and the adjacent land with moisture. If they do not increase the rainfall, then perhaps the land which is now being covered with woods can be more profitably used for wheat. Again: the welfare of much of the eastern United States, now well watered, may turn in the future upon the decision of this question. In some parts the forests are being cut away, and thereby the rainfall may be reduced to such an extent as to make the soil unproductive, in which case the timber-cutting should be stopped in time.

From this the economic point of view, it must be recognized that to be of any value, the influence of woodland upon rainfall must be considerable in amount. A minute influence is equivalent to none at all, so far as economic effect is concerned. If it should be found that woodlands induce only a trifling modification in rainfall, the solution of the question is substantially in the negative, viewed economically. In our examinations of records and other tests of the comparative amount of rainfall under differing conditions of forest-covering, we are, then, to look for changes of considerable magnitude. The variations in rainfall from year to year and from place to place are great, — so great as to mask, in a limited series, or in observations at a few stations only, any general change. It is, of course, understood that the difficulty in the way of detecting the general movements of rainfall lies in these temporary and local fluctuations, and it is apparent that to eliminate them it is necessary to use what Dr. Fernow aptly calls the wholesale method, to bring together into the investigation large numbers of observations, from many stations, scattered widely over the territory under examination. I cannot conceive any retail method that will yield a result worthy of any confidence, as is shown by the fact that it is very easy so to select stations and years of observation as to obtain any desired result.

Of the retail methods of investigation in use in Europe, cited by Mr. Fernow, that of pairs of stations, one situated within the forest, the other 100 metres outside it, seems to promise no result; first, because it is a retail method, and, second, because if the forest has any influence, it must, in order to be of any value, be felt more than 325 feet away from the margin of the forest. We cannot afford to cover the land with woods in order to increase the rainfall. We must have some land to cultivate. The conclusions from the observations in Bohemia, cited by Mr. Fernow, can only be misleading. To compute from the rainfall in the open valley, and from an estimate of the rate at which rainfall increases with elevation, the theoretical rainfall upon cleared mountain-sides, and then to conclude from the discrepancies between these results and the observed rainfall upon the timbered mountains that the forests have had a certain effect upon the rainfall, is a case of theory run riot.

I know of but two attempts to use the wholesale method, both of which were mentioned by Mr. Fernow, — that of Mr. Harrington and my own. Mr. Harrington's method consists in a comparison of two rainfall maps made from data of different dates, — the Blodget map, made in 1857; and the Denison map, made in 1844. The two maps are not strictly comparable, as the first purports to show areas of equal rainfall, while the last shows lines of equal rainfall. Nevertheless, the former may be made rudely comparable with the latter by means of certain assumptions regarding the relative positions of these lines and areas. Mr. Harrington's examination was confined to the supposed increase of rainfall on the plains. Finding that the isohyetal lines of 20, 25, and 30 inches were in some places slightly farther west on the Denison than on the Blodget map, he concluded that the rainfall has increased.

What is the value of this evidence, and, first, of what authority are the maps? Upon the Blodget map I find only five stations in the entire area of the plains, north of Texas; viz., Forts Riley, Leavenworth, Atkinson, Arbuckle, and Kearney. The only data in this area of nearly half a million square miles consists of the observations at these five stations. It may be safely said that the rainfall-curves in this area are at least 99 per cent hypothetical. They might as well be drawn a hundred miles on either side of the position assigned them by Mr. Blodget, without contradicting the observations. Their position is necessarily based almost entirely upon Mr. Blodget's judgment, and not in any appreciable degree upon observational data. The Denison map is better. The worst that can be said of it is that it is a popular map, made to sell. But the weakest link in a chain limits the strength of the chain, and the Blodget map is the weakest link in Mr. Harrington's chain of evidence. With it his conclusions must stand or fall, and, as has been shown, this link is most absurdly weak.

Let us look at the matter from another point of view. If Mr. Harrington's conclusions regarding the rainfall on the plains, drawn from a comparison of these two maps, are correct, similar conclusions regarding the rainfall of other parts of the country must likewise be correct, especially as the data upon which the map is based are elsewhere more abundant, and the maps correspondingly more reliable. Let us see what other changes are shown by the maps to have occurred. About Cape Hatteras the rainfall has apparently increased from 48 to over 70 inches; in southern Louisiana, from 45 to 60; in northern Florida, from 50 to 65; in the mountains of North Carolina, from 36 to 48; and so on. It is unnecessary to specify further changes, as there is scarcely any part of the country in which, if this method of reasoning be correct, great changes in rainfall have not occurred between 1857 and 1884.

The method employed in my investigation of this question, and the results obtained, are set forth in an article in *Science* for Jan. 6, 1888. The explanation there given seemed to me to be sufficiently clear for the average reader. It appears, however, that it admits of being misunderstood, and has been misunderstood by Mr. Fernow. I will therefore state it once more, and with greater fulness. The method used is a wholesale one. Certain areas in this country, of great extent, in which the changes in respect to forest-covering have, within recent years, been radical, were selected, and an examination was made of the rainfall measurements in these areas during the time of foresting or deforesting, in the hope, not of obtaining a quantitative expression for the influence of forests, but of learning whether they have appreciable influence. One of the areas selected was the prairie region, where it is well known that during the past fifty years the wooded areas have greatly increased, — so greatly as to change the whole aspect of the country. This increase of woodland has been a progressive one, going on gradually year after year. Now, if increase of woodland increases the rainfall, it follows necessarily, that, barring its sporadic fluctuations, the rainfall also has increased progressively in this region. The following, then, is the proposition to be proved or disproved by the rainfall records: that the rainfall has increased, and that progressively, in the prairie region during the past fifty years, as foresting has gone on. Within this region I had access to the records of twenty-four stations scattered widely over the area, each station having a series of records of considerable length, ranging from ten to forty years. These series are scattered over the past fifty years in an irregular manner, and no attention was paid to the particular years which each series embraces, as it is not believed that it is a matter of any importance. The series from each station was cut in halves, and each half added, giving the total rainfall of each half. Now, were there no sporadic fluctuations, — in other words, were the rainfall regular in amount, — the comparison between the sums of the halves of each series would be sufficient to base a conclusion upon. If the rainfall had increased, the earlier half series would be less than the later half. As a matter of fact, however, these individual results are very discordant, owing to the irregularities of rainfall; and it is necessary,

in order to get rid of these irregularities, to get together a larger number of observations. This is done by simply adding together all the first halves and all the second halves: that is, in this case, I have added columns, etc. As I understand it, exception is taken to this operation, as bringing together quantities which are not homogeneous. Suppose that, instead of adding up directly each half of a series, the mean rainfall at a station is obtained from the whole series. Now, if the proposition as above stated be correct, this mean rainfall is, barring irregular fluctuations, the rainfall of the middle year of the series. Let the residuals be taken. Is there any impropriety in adding up the residuals, not only in each half-series in one sum, but those of all the half-series, for comparison of the sums of the two half-series?

Or, to put it in mathematical form, let R equal the mean rainfall of a series, which is equal to the rainfall of the middle year, r the rainfall at any time, t the interval in years before or after the middle year (plus when after, and minus when before), x equal the rate at which the rainfall is supposed to increase, which may be assumed as constant over the area, as it is a qualitative rather than a quantitative result which is sought. We desire to learn whether x has any considerable value. Then

$$r = R \pm tx, \text{ and } x = \frac{r - R}{\pm t};$$

and, for a single series,

$$x = \frac{r_1}{t_1} + \frac{r_2}{t_2} + \frac{r_n}{t_n} - \frac{r^1}{t^1} - \frac{r^2}{t^2} - \frac{r^n}{t^n},$$

the mean rainfall R being eliminated: x being the same over the entire area, and the mean rainfall being eliminated, the above equation applies to all series, and they may be properly combined for the purpose of obtaining the value of x, and

$$x = \left[\frac{r_n}{t_n}\right] - \left[\frac{r^n}{t^n}\right].$$

As has been stated, this method was used to test the above proposition, in the prairie region. Twenty-four stations were used, and the observations of 428 years were used in evidence. The result showed that there was a trifling amount *more* rain in the earlier than in the latter half of the series. In short, it showed that the rainfall had not increased.

It was applied in Ohio, which from a forested area has become with settlement mainly a deforested area. Under the terms of the proposition, the rainfall should have diminished, but the amount of the diminution is trifling, being but .21 of an inch per year. To this result twelve stations, with 294 years of observation, contributed.

Southern New England, comprising some 20,000 square miles, was originally a densely forested region. With the progress of settlement it was almost entirely cleared. In recent years, say since 1860, a reverse movement has been going on. The competition of Western farms and cheap transportation is driving New England farmers to other vocations, or is forcing them to move to other parts of the country. Thus the farms are being abandoned, and are growing up to woods. To-day Massachusetts contains 52 per cent of woodland, and Rhode Island even more. Southern New England, then, presents two phases of change for investigation. During the earlier period, with the cutting-away of forests, the rainfall should have diminished, while during the past twenty-eight years it should have increased. During the first period there were used in the investigation eighteen stations, with 400 years of observation. The examination showed that the rainfall had *increased* while deforesting was going on.

In the second period fourteen stations were used and 200 years of observations. The examination showed no change whatever.

This investigation has convinced me that forests exercise no influence whatever upon rainfall. I wish to state this plainly, as it was suggested at the last meeting that I had some doubts concerning the results obtained. I regret that any thing in my paper should be capable of such a construction, as it was certainly as far as possible from my thoughts.

I am aware that this conclusion is at variance with the popular idea, and that a popular idea is not a thing to be disregarded, as there is usually some reason for its existence. We find woodland and a heavy rainfall generally co-existing. In almost all places enjoying a heavy rainfall, the land is covered with forests, unless they have been removed by man. It may be that in this case an effect has been mistaken for a cause, or rather, since it is universally recognized that rainfall produces forests, the converse has been incorrectly assumed to be also true.

Although forests have no influence upon precipitation, yet they do exert a certain economic influence. Without increasing rainfall, they, in common with other forms of vegetation, economize that which falls, retaining it somewhat as a reservoir, and preventing its rapid descent into the streams. In this way, too, forests tend to reduce the magnitude of floods and to regulate the flow of rivers, thus preventing disaster and improving navigation. This retention of the rainfall is, however, accompanied by a rapid evaporation from the leaf surfaces of the forest, whereby a considerable proportion of the rainfall returns to the atmosphere without reaching the earth. On this account it is urged, and I think with reason, that in our arid region, which is dependent for irrigation upon its streams, it is advisable to cut away as rapidly as possible all the forests, especially upon the mountains, where most of the rain falls, in order that as much of the precipitation as possible may be collected in the streams. This will cause, not a decrease in the annual flow of the streams, as commonly supposed, but an increase, coupled with a greater concentration of the flow in the spring months, and result in rendering fertile a greater area of the arid region. It may be added that the forests in the arid region are thus disappearing with commendable rapidity.

There is no question but that forests reduce the extremes of temperature in their immediate neighborhood. They also serve mechanically as windbreaks, diminishing the force of air-currents. In these and perhaps other ways they serve a useful purpose.

But with all this in mind, is it worth while to go on planting trees for their climatic effects? It seems to me, that, apart from the uselessness of it, nature is planting trees at an infinitely more rapid rate than man. For every tree planted under the timber-culture act, or on Arbor Day, a thousand spring up of their own accord. Every deserted farm east of the plains grows up to forest. Half of southern New England is to-day wooded, and the proportion is increasing every year, and yet in Massachusetts they have every year an Arbor Day, when the farmers turn out and solemnly plant a tree apiece.

MENTAL SCIENCE.

The Psychology of Deception.[1]

THE deceptive character of the evidence of the senses has become attributed to them because of the failure to recognize that we seldom have to do with a simple sensation. What deceives is not the information of the sense, but the wrong interpretation of this information by the mind. Such interpretation need not be conscious, and often is not so. The familiar experience of raising a pitcher of water, usually well filled but upon the present occasion empty, and finding it dart upwards in our hands, is a case in point; for it shows that we estimate the amount of force necessary to raise the pitcher, but only become conscious of this inference when it happens to lead us astray. The phenomena of the stereoscope abound in illustrations of such unconscious reasonings. One of the simplest types of deceptions arises when such an inference, owing to an unusual disposition of external circumstances, leads to a conclusion that better evidence shows to be false. A ball held between two crossed fingers seems to be double, because under ordinary occasions an impression on the right side of one finger and on the left side of its neighbor (to the left) could only be brought about by the simultaneous contact of two objects. Everywhere, then, we interpret the unfamiliar by the familiar, the unknown by the known: illusion arises when the objective conditions change their character, and real deception occurs when this change is not recognized, when no better evidence is present to antagonize the false inference. The child who regards a spoon half immersed in water as really bent,

[1] See an article with this title by Joseph Jastrow, Ph.D., in the *Popular Science Monthly*, December, 1888.